

CREATIVITY RESEARCH JOURNAL

Volume 7, Numbers 3 & 4, 1994

SPECIAL ISSUE: CREATIVITY AND DISCOVERY IN BIOMEDICAL SCIENCES

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SPONSOR: ROYAL SOCIETY OF MEDICINE, LONDON

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Does not include major discussion.

Introductions to the Meeting of the Royal Society of Medicine

Sir Christopher Booth: *This meeting was organized by the American Foundation in New York. We are very grateful to Bryce Douglas and Nick Christie for all they have done to help, and to Bill O'Reilly, who has done all the hack work in putting things together.*

The origin of medical societies in London goes back to the 18th century. You remember that we have in this country Royal Colleges, which are our formal professional bodies, and the medical societies developed in the mid 18th century to provide a forum for discussion of medical affairs in a general sense, separate from the Royal Society which, at that stage, was the main forum for scientific discussion.

The first was founded by Benjamin Franklin's physician in London, John Fothergill. You can see his portrait hanging in a place of honor—the Gilbert Stuart portrait—at the top of the stairs in the Academy of Fine Arts in Philadelphia, right beside Benjamin Franklin. His society was a society of physicians, and he was, in fact, a foreign member of the Philosophical Society in Philadelphia, one of the earliest of the foreign members. So was his pupil, John Coakley Letsom, the man who founded the Medical Society of London in 1773.

The Medical Society of London was the first society in London to bring together, under the same roof, surgeons, physicians, and apothecaries, the three branches of the profession. But it went into some difficulties, because a man called Sims insisted on re-

maining president for 21 years. That is quite a long time, and in our generation, the only man who has been criticized for being president for a long time was the late Russell Brain. When he became president of the Royal College of Physicians for the seventh successive year, there was a very critical article on the subject by the then editor of the British Medical Journal, who wrote a marvelous leading article entitled "The Gold-Headed Cane." Anyway, Sims was president for 21 years, and everyone was rather fed up with that, and so a group of people split off and founded a new society, which they called the Medical and Chirurgical Society of London, in 1805.

In the early years of the first decade of this century, this society got together with a lot of other smaller pathological and physiological societies in London, and they all came together to form the Royal Society of Medicine. I stress this just simply to imply that the Royal Society of Medicine, unlike most other bodies in London, is a comprehensive organization that brings in people from the widest spectrum in medicine and medical science, and we have a very strong open section, which brings us into contact with the public.

In fact, we have over 30 sections within the Society, all covering a wide variety of things. This is why I think it is particularly appropriate that this meeting should take place under the aegis of a society that looks broadly—which is interested in bringing to-

gether people from different disciplines. This is at a time when one has to remember what I term "the Kornberg paradox," which is the point that Arthur Kornberg made: Whereas in clinical medicine where I belong, all the specialties have been diverging, in science, everything is coming together, because of the modern molecular language. This molecular language is bringing together everybody from different disciplines.

Joshua Lederberg: A number of people, when we were recruiting them for participation, were a little bit alarmed about that word creativity. Harriet Zuckerman in her paper has a large bibliography of criticisms of efforts at the analysis of creativity. I would like to stress that our hope is not the generation of a grand theory of the creative process. Rather, we

hope to open up a less ambitious but more realistic examination of the history of discovery, with concrete examples, and with the benefit of the very participants as part of their account. Our aim is to record more authentic information about just what happened at certain seminal moments (or intervals) in the history of science. We will then be in a much better position to spin our philosophy, our psychology, our history, and our social, behavioral, and humanistic understanding of what has been involved in creative discovery. We would really like to get down to the historic facts. We seek not any closure on our theoretical perspectives on scientific creativity, but a beginning. And if there is even one useful gestation that began and was fertilized here, we will feel that the RSM meeting was worthwhile.

not through trying to falsify my or other people's hypotheses, but through picking up and following a chance discovery, and through exploring it as thoroughly as possible, until finally it was time to leave the subject to the next generation!

Discussion

Joshua Lederberg: *Katz's paper prompted me to ask myself what I thought about Popper, for better or for worse. It was fairly late in the day that I read him, and by then I had already formulated a practice of how I did research. Although some of it did seem to ring true, I still don't know how seriously to take the doctrine in the way that Peter Medawar and others have elaborated it. In my own experience, there have been some very sharp confrontations where falsification was the issue. I would work very hard to identify the critical experiment that would enable one to decide whether to proceed with a given hypothesis or not, but on other occasions there are other motives. I don't know how to read what is attributed to Popper, "that the only function of an experiment is falsification." Does this mean that is all that can be logically analyzed? Or, as Medawar may be seeming to say, that individuals should be discouraged from doing experiments that are not designed around the falsification paradigm? If so, that will knock out a very large part of the total experimental effort!*

Most theories of scientific method that I have seen don't operate in the real world; they are concocted by people who have not actually had to solve a problem in confrontation with nature, and whose ideas are just much too neatly packaged. They would imply a linear progression from a datum to the induction of a theory, if you believe that, and then to the conduct of the critical experiment, and that's the end of it.

I could find no better simile than that of

the epicycles, which used to be our explanation of planetary motion, to describe what really happens. Lots of fits and starts, a new datum may overturn your existing conceptual framework. You go back and try to develop another one. You may be simmering for a long time without really knowing what you are doing. You may be going around the circle starting from the top and going clockwise, in a fairly smooth fashion, and then something happens and you regress, and you start all over again. Or you may decide the whole project isn't worthwhile, or your funding may lapse. Or you may run out of a critical research material, so that research very rarely is as monotonically linear as what's given to be the Popperian model would support.

Last night, in preparing an introductory reaction, I tried to think of the range of motives that had inspired the experiments I had done. I don't want to discount falsification. It's very exciting when one can think of a critical experiment that has the possibility of falsification, and some of my most important work has been conducted on that paradigm. Then, the very next day, there would be a discovery of some unexpected phenomenon that would throw you on the scent of another trail.

So, besides experiments for falsification of hypotheses, I have to say, sometimes the motive is not to falsify an hypothesis, but to discredit an adversary, or to discredit a school. And the issue isn't so much one single experiment, one single idea, as it is a whole framework of presentation that others had offered. But just let me quickly list some of the others. You mention amusement. Yes indeed, to play can be a lot of fun; there can be kinds of experiments where just the sheer conduct of them is a pleasure, and I think play is a very appropriate source of creative impulse.

It isn't always a totally rational activity.

Very closely connected with that is elementary curiosity; "What is going to happen if I mix A and B?" That would be stretching the notion of hypothesis very far indeed. When you get a novel reagent, or a novel instrument, very often you have a sense that there may be nothing better to do than just try to mix up two compounds, or to apply a physical measurement in a situation that hasn't been done before and see what happens. You may be floundering, you may be immersing yourself, you may be fishing, in hopes that some new data will emerge, or some new ideas will come to you just in the process of that immersion and concentration. This is not a falsification of a defined hypothesis; it's trying to constrain the cosmos so that you can focus your attention on some tinier piece of it, from which perhaps more specific hypotheses will emerge.

A lot of experiments are done to develop new tools. One sense of frustration about not being able to make progress in a given area is so high that, instead of trying to answer a question, you decide you had better sharpen the tools. And that in turn enables yourself, or others, to go further. Or you may just want to exercise a tool or an instrument (if for no other reason than to justify the costs that went into producing them; having them stand idle is even more reprehensible than applying them to what might seem to be frivolous activities). Or to display your virtuosity; that's part of the fun of experimentation—to be able to have the dexterity, the ingenuity, the ability to consummate a manipulative function that you can do better than anybody else.

Or you may want to improve your skills so that you can reach that high plane. And very often, you do experiments when you know what the outcome is going to be; you have already done all the work, but it just isn't appropriate yet for presentation. You did them in a somewhat incoherent fashion, you did an experiment with 16 or 18 irrele-

vant variables, and if you are going to report it accurately, you would have to put them all down in the tables and so forth, and it's just easier and better to do it all over again, but in a highly constrained and simplified fashion, to concentrate on the point that you are going to make for publication.

So there are innumerable motives (and I may have left out still others) for why one actually conducts a given experiment. But I'd like some of the professional philosophers to respond and tell us, "What does Popper really think about that?" Is he really as narrow in his view of why we do, or should do experiments, as his followers have indicated?

Thomas Nickles: *I think it is correct that Popper took a quite narrow view of the role of experiments. I would agree with just about everything Katz said. But as for the many roles that experiments can play in research, I would like to reflect the question back upon our distinguished scientists. As a philosopher, I myself went through a Popperian phase when I was in school. At that time I was a very ardent Popperian. I later worked my way free, but I am struck by the number of people on this panel who have already mentioned Popper in one connection or another—sometimes to criticize him, but often to praise him. Coming from the other side of the Atlantic, we are sometimes puzzled by the reputation that Popper seems to have in certain circles here. He is a very eminent man and has had many seminal ideas, there is no question about that. But the sort of veneration that Sir Bernard mentioned is puzzling. So I should like to ask those scientists who mentioned Popper, how did you first hear of him, and in what connection? Did you hear it from someone else, or from general reading? And what was your first reaction to his ideas? Did this contact with Popper's ideas come early enough in your career that you think they did have some influence, positive or negative, on your work?*

Howard Gruber: Suppose we grant that having a hypothesis that actually controls what you are doing at a given time is not the only mode in which scientists operate. In my reading of Darwin's notebooks, it was quite rare that he was actually testing a hypothesis. Suppose we grant that much, a next question one might ask would be, "If you do have a hypothesis, what kind should it be?" What kind of test should you run? Should it follow the confirmatory strategy, or the disconfirmatory strategy?

Here I want to introduce an interesting convergence of experimental and historical evidence. Ryan Tweeney, a psychologist in Ohio, has done some experiments with living scientists, showing that sometimes they have confirmatory biases and sometimes they have disconfirmatory biases. He has also done some interesting work with Faraday. He went through Faraday's notebooks and identified those moments when Faraday did seem to be testing a hypothesis. In Faraday's case, I think, this happens more often than in other cases; he was very orderly in that way. In the early stages of a given line of work, he wanted to get hold of his phenomena. And that is when you need to have a confirmatory bias, Tweeney argues, and this seems reasonable.

In a later stage, when you think you know what you are talking about, that's when you might want to turn around and try to disprove your own ideas. I don't mean to suggest that there is a simple sequence from confirmatory to disconfirmatory biases. Another point has to be added: that one man's confirmation is another's disconfirmation. In the rivalry/putting-down-your-adversary point that you made—it's not clear how you define confirmation. Larry Holmes' comparison of Lavoisier and Krebs is interesting in this regard too, because he suggests that Lavoisier could afford to work in one mode and Krebs in a different mode. And, of course, two centuries separated them.

Lederberg: Well, I can recite from my own experience that I've had two kinds of thrills in doing scientific work. One of them is that culmination when you feel you are at the point when you have got a critical confrontation, and you can do that critical experiment that has the possibility of falsification. I would attribute great strength to that paradigm at that stage in the development of the scientific effort. The other thrill is when you run into a paradox. You have not only a surprising datum, but one that seems to be in conflict with what you had thought about before. And when you can get that sharp contradiction, it's much happier than just a random datum that's a surprise, because now you have some clue about how to set up further experiments.

To answer the question about the history of first encounter, just to give one datum, I took philosophy in school, but it was with Ernst Nagel. What I remember of his teaching, probably more than from his writing, was that he was fairly eclectic in the way that he taught the philosophy of the scientific method, and left one with a certain respect for dialectic process, a certain skepticism about whether any approach would really be a complete and sufficient explanation about how we do things. I guess I still reflect that eclecticism today. I didn't encounter Popper until I'd been 15 years into my own scientific effort.

Sir Andrew Huxley: In answer to Nickles' question of how we got interested in Popper: As Bernard Katz said, in my case it was through the influence of Peter Medawar and Jack Eccles. I remember particularly a lecture by Medawar, and Medawar was the proposer of Popper for Fellowship of the Royal Society. My interest was also stimulated by Eccles, both in conversation and through his books. Only a few weeks ago, I was sitting next to Eccles at dinner, and we got into a

discussion, indeed, a slightly acrimonious debate, on this question. He asked why I was critical of Popper, and I said that it seemed to me that things seldom went according to his principles. Eccles said exactly what Bernard has just said, that he thought the principal point of Popper's ideas was the willingness to give up a formerly cherished idea. I think that is not a correct interpretation of Popper.

Eccles went further, and said that the turning point when he switched from electrical to chemical transmission, was when some experiment seemed to receive a much better explanation in terms of chemical transmission than of electrical. Now that is not at all Popper's proposition, and I said so to Eccles: Popper's proposition is that you make progress by an experiment that disproves something. And here was Eccles positively asserting that the thing that had switched him from electrical to chemical transmission was something that made chemical transmission easy to appreciate and to use as an explanation. It wasn't that the experiment demolished electrical transmission, which would be the Popperian way of progressing, it was that the experiment suggested a different type of explanation. And I would entirely agree with this, but it seems to me that it is not at all a la Popper. So here is another paradox.

Lederberg: What I am finding is that Popper may stand next to Thomas Kuhn in our pantheon of philosophical poets, and that their great popularity is their wonderful projective artifice. As with holy scripture, people read into them what they would like to understand or perceive, or get confirmation from. That may be a wide variety of interpretations, far beyond what they had said. That poetic function is not to be dismissed.

Lord Butterfield: I am very intrigued that our opening speakers—we have heard one, we're going to hear Andrew in a moment—really

are talking about discovery and the excitement of discovery, whereas it strikes me that Popper isn't involved in that. He is involved in trying to sort out how you then work on your hypothesis to find out the facts. I got into Popper because I have a son who is a philosopher, and he arrived with a little Penguin book, threw it down, and said to me, "Pop, I think you ought to read Popper." He was particularly pointed in this, because he knew that I was mingling with a lot of epidemiologists, and I do think that Popper has a strong message for people who try to discover facts by association. There is the story of the black swan, you will remember. Until somebody got to Perth, all swans were white. When black swans were found in Western Australia, that hypothesis didn't hold. Heaven knows what would happen to many of the epidemiological theories if you could knock them down whenever somebody finds a case that doesn't fit. For example, a long-surviving member of a family with a high blood cholesterol! I am not going to go into that here, but I do think I must give some tribute to Popper for introducing a set of critical ideas that I believe have been valuable for people whom you might say aren't scientists in Bacon's sense in that they can't test their hypotheses by perturbation. They are more truly observers.

Frederic Holmes: I think this attention we are giving to Popper is all of interest, but I am a little concerned that we may be missing an opportunity to draw a little more out of what we can learn from Sir Bernard's own experiences. I would like to go to the statement, which you pass over lightly, that this unexpected discovery you talk about provided many years of serious occupation and entertainment. So I was led to go back to your paper of 1958, where you discuss this, and had by then the hypothesis that these potentials were reflecting the discharge of

acetylcholine. And at the end of that paper you wrote, "Now all this is at present no more than the mere ghost of a working hypothesis and we cannot even hold out a promise that no one will be able during the next few years to obtain decisive experimental refutation or support for this idea." Well, could you say a little more about how you arrived at that hypothesis, how you constructed it and the interaction between it and the experiments you were carrying on in those years?

Lord Adrian: *May I take up what [Katz] said about the extreme enjoyment and fun of scientific work, with which I agree entirely. I hadn't heard the story of A. V. Hill saying we don't do it because it's useful, but because it is amusing, and his then being well received by his audience. I believe now, at any rate at a political level, that would be very ill-received, though it doesn't seem to me to have changed in the perception of the scientific world. A lot of what we do is because of curiosity and the fun of it. But what has changed very markedly is the political and public perception of the justification for a lot of very expensive people doing something for their own entertainment. To put it as crudely as that. I wonder how we are going to either change the perception of politicians or justify what we do. I mean the difficulty is that we know perfectly in ourselves that fun is a major part of why we do what we do, and it does succeed in producing a very great deal of extremely important stuff. But the politicians currently find this very difficult to understand and very difficult to accept. What I perceive is that we aren't being successful at making the case, that's what worries me very much.*

Lederberg: *Well, I think we need to distinguish two things, the motive of the individual investigator is pretty much irrelevant to the social motive of why research ought to be supported. If it turns out that you and I can*

have a lot of fun during research, and by that enjoyment make less of a claim on other social resources than we might do otherwise, then I'll say so much the better! So long as the net outcome is as productive and effective as I think the evidence is clear it has been. So I don't think one ought to connect the motive of the individual with the social yield; they may be totally disparate. Most of the people 'round this table would say that when you are wandering around in the dark and trying to discover things of great fundamental import and which are totally unpredictable, that you had better have some fun in the process or it will never get done at all.

Baruch Blumberg: *I personally have found Popper's "method" very useful, but in a limited sense. It doesn't deal with the complexity of hypothesis formation, which is the daily experience of the working scientist. I was initially introduced to Popper by reading Bronowski, and later through Brian Magee's handy precis, which he wrote when he was at Balliol College. Much of the value of Popper comes from a consideration of the deductive phase of scientific process, and very little has to do with the inductive. It is the latter phase whose formal exercise may generate new ideas.*

Kenneth Schaffner: *I wanted to add another comment on Popper, because I think it would be useful to put some of the discussion in some philosophy context. Tom Nickles led off by saying that he had had a phase that he had gone through where he believed in Popper, and I think I probably spent a similar 12-month phase. I had been educated at Columbia by Ernest Nagel and, due to the influence of Kuhn and Feyerabend, began to have some questions about the adequacy of the logical empiricist approach. And Popper looked like a solution, so I taught some of my first courses from his book *Logic of Scientific Discovery*, and I talked to some colleague scientists who also believed themselves*

to be Popperians. I found out that most practicing scientists had read through the first two or three chapters of the *Logic of Scientific Discovery*, but had never gone on to read the chapters on simplicity, or particularly on corroboration. And I think that those are important chapters to read, and I think it's also important for us to remember, in the context of our discussions about discovery, just where limitations arise in Popper's work.

One of the problems, which I don't think he has ever adequately addressed, is known as the Duhemian problem, and that arises when we have other hypotheses that we accept as background hypotheses. We then find a falsification of a particular hypothesis under test, but the reason for the falsification is that there is some problem with the background assumption. That is something that doesn't neatly fit into the Popperian system, and a number of critics have suggested that it can't. But it's very important with respect to scientific discovery, because it is critically those issues that require us to reevaluate those taken-for-granted assumptions and modify them.

The other important aspect of Popper that relates to scientific discovery has to do with his doctrine of corroboration. This is a fairly esoteric doctrine, and I think that even some Popperians really don't understand the implications of it. But, to put it very briefly, and following some suggestions of Wesley Salmon, Popper's logic suggests that all we deal with logically is *modus tolens*: we reject the hypothesis on the basis of some particular observation. But *modus tolens* is a rule of logic, and it doesn't carry any kind of inductive weight whatsoever. If you want to get inductive weight, and you want to attribute it to corroboration, then you've got inductive logic and all of the problems that Popper wanted to outflank.

Without corroboration viewed inductively, the analysis is, from a logical point of view, empty, which suggests either that most

philosophers of science have been completely wrong in understanding what Popper is about, or that there is some limitation in Popper that takes us in the inductive direction. This is a direction in which we need to go so that we ought be able to entertain hypotheses, think of them as possibly true and such—an attitude that moves one into the inductive realm. So I think that, both with respect to falsification and the Duhemian complications, as well as with respect to the need for some kind of an inductive logic, you won't find that Popper's work is any kind of a complete answer.

Lederberg: Ken Schaffner, I think you'll know the case I am referring to when I say that one of the great difficulties a scientist faces when he has falsified something, is knowing exactly what it was that is being falsified, which I think is very similar to your point about background hypotheses. An event I can recall with the greatest chagrin, is having walked right up to a very significant discovery and turning my back on it because of the misunderstanding of just what it was that had been falsified in that current context. It's very easy to throw the baby out with the dirty bath water.

Howard Gardner: The discussion suggests that it wasn't the ideas *per se* that originally attracted people to Popper. Possibly our connections to fields outside our own are motivated by different kinds of factors. The first thing that Katz said was that there was a psychological reason why Eccles was attracted; namely, he wanted to justify mistakes he made. Would that that was the only reason to attract—it's easy to make mistakes! Most of the other people mentioned sociological reasons. They read Popper because Medawar had read him or because Eccles had read him and, if other people were reading him, one should read him too. And then when somebody said, "Why all this fuss about

Popper?" *I think the answer is in part geographical. If you're in a country, and there's one philosopher of science who stands above the crowd, one should read him. Thomas Kuhn occupies that role in the U.S. It seems to be a really different motivation than what would attract one's interest within one's own field. Blumberg was the only person who seemed to have really tried to use Popper's ideas to sort out his own research program.*

Blumberg: *I found Popper's concepts consistent with our prior experience. It helped me to formalize the approach we were using and provided it with a kind of validity.*

Carleton Gajdusek: *I'm rather confused by having a subject of "creativity," and then launching off into a discussion of what, in my opinion, is a very late stage of creative research, after a great deal of creativity is finished—namely, the testing, be it confirmatory or disconfirmatory, of a specific hypothesis. There was a little mention of fun and play; these are the maxims on the wall for my 40 years in a laboratory; that's all we've ever done, to have fun and play, and all we ever will do. I agree with that concept. But then the big problem comes up—where did you first get that idea? When was the generation of that creative thought?*

In my youth, hanging around Linus Pauling and the Caltech crowd, ideas were cheap, and you had a thousand hypotheses for any unknown process. Any group of graduate students in Geology at Caltech could generate almost every conceivable possible idea in one afternoon, faced with an unknown such as multiple sclerosis, schizophrenia, cancer, or the planets around unknown suns. And then we say: "Where do these ideas come from?"

I can read Lucretius and find everything that Niels Bohr ever turned out in terms of ideas. The Pythagorean and Zenoist controversy in early Greece framed the argument

of the sacrosanct integers vs. the innumerable continuity. And so the question is, "What happens when you have creative ideas?" They are all around us, and any intelligent group of people can spout them forth. You certainly don't go testing them all, ad infinitum. You certainly don't have them all quantitatively formulated. You need observation, you need data. It's this matter of constantly looking at data for any problem like diabetes, multiple sclerosis, rheumatic fever, cancer. Who in the world has one idea, one hypothesis? It's ridiculous—you have 50 in the back of your mind. You look at data, and look at data, trying to see which associations lead you to be able to frame any further step.

So when the philosopher of science asks us about the idea that's working, "When did it first click?"—it's the total wrong approach to understanding how creative discovery was made. Ideas usually come from three thousand years ago. The question is, "When did an association, or a paradox in the data, make one of those multiple ideas further tenable and investigatable?" And so, I would never ask the origin of the idea. If I am tracing Darwin's or Newton's ideas, I can go way back before them.

But in fact, most of the ideas are all circulating today and it's the question of who will give us the remote possibility of producing a Popper experiment, confirmatory or disconfirmatory, or getting more data that will put us in another line and grab from this wealth of hypotheses one we can practically investigate. Design the quantitative experiment and pick the correct form to quantitate them. And that's a different story than what we are talking about, from my point of view.

Lederberg: *I don't think anyone will disagree that confirmation or disaffirmation is a late stage. But I'm not sure whether you are affirming or contradicting the view that the critical function is a very important aspect of*

creativity. It's not enough to spin out every possible combination of words, and say that all those sentences exist, and they certainly exist in potential. There's also the ability to use existing knowledge to try to refine which of those are worth further pursuit. And that's the critical function.

Gajdusek: If you were making a creative machine, you would need an increasing data pool developed from different observational directions against the possible hypotheses—not one hypothesis. And you certainly wouldn't have just one critical test—the one we later publish. But I think that's what we are all doing most of the time. And then some philosopher says, "When did you come to that idea?" It's usually at a level of having had hundreds or at least dozens of ideas, and finally getting some data that points toward one. Then we put it forth at that moment as though it were the new idea. All the others less favored by the data we abandon in their infancy and don't publish them.

Lederberg: But that's the idea. It isn't just the hypothesis, it's how you would go about distinguishing it from all other hypotheses to make it worthwhile being the subject for further pursuit.

Sir Raymond Hoffenberg: I just wanted to revert to Sir Bernard's earlier statement about having changed from the shutter hypothesis to the vesicle hypothesis, and also something Sir Andrew said. From their comments, it would seem that progress was made because a change of hypothesis was introduced. In the first case, I think Sir Andrew's case, because a new, more convincing hypothesis had been generated, and in Sir Bernard's case, because the old hypothesis had been falsified. So it seems to me that both processes are important and both can be deliberate. We also heard from Barry Blumberg; how he deliberately used Popperian ideas in order to falsify, or to try to falsify his hypothesis. So

the objection to Popper is not necessarily that he is wrong, it is really he has overstated the case and came down entirely on one side. I just wondered if that interpretation is acceptable?

Michael Sela: I want to refer to the fun that has been mentioned, because I am a great believer in fun from research, but I think it sounds a little bit frivolous. Actually I think that fun goes together with perseverance in research, and I think that fun is a very important component for the sake of perseverance; it's necessary. Now, if we move to the problem of the individual scientist versus the social effect and social needs, I think the problem is to explain that, in order to have practical results, you must give the fun to the individual scientist. I also have a question, mainly for the philosophers and historians of science: Would you recommend to a young scientist that it is important, or even relevant, for the quality of his research, that he should read Popper, and should go into this in depth before he starts on his experiments?

Lederberg: Well, I don't know if it would be Popper. But I think some sense as to the canons of proof, and some overview about how other people have gone about the conduct of experiments, the selection of strategic issues, and how one reaches conclusions in science, would be advisable. It may have to be something larger than Popper to be able to frame that. Otherwise, you are expecting a kind of intuitive transformation, that the stuff that you and I already know from years and years of experience is suddenly going to pop up, without any basis for that transfer. So, implicitly or explicitly, one does have to teach a philosophy of science at some level to students. But it may not always be that explicit.

Nicholas Christy: Two comments. The first is probably obvious to everybody here. It occurs to me that most of our discussion this

morning has been an attempt to falsify Popper's doctrine. This to me has been useful, since I find that my own temperament is antithetical to what he says; I think that the process in scientific discovery is indeed eclectic and comes about by many pathways. I would like to bring to the discussion the generation of poetry, which is not so unconscious and so obscure perhaps as we think. And if I may say it, I think too much has been made about the sharpness of the difference between these two processes, the scientific and the poetic. I think there are many similarities. It just happens that, over this weekend, I stumbled across some books in the Athenaeum Club library, where there are dozens of books I wanted to read, but I couldn't reach them because they are about 90 ft up in the air. But such as I could lay hands on, were commentaries on Spenser, Coleridge, and Eliot. I don't know why I hit upon those; except that they were reachable.

The first had to do with Spenser, a person in poetry who is like the scientist in that he sees things more sharply than just anybody. I'm not talking about God-given gifts, but then again I am, because people who are capable in science, in fact, see things differently. I found the work on Spenser phrased it quite aptly. In a discussion of Spenser's temperament, the commentator wrote about Spenser's lucidity; Spenser possessed a clarity, he said, like that of a "terrible crystal," that is, the kind of unusually keen insight that characterizes both the poet and the scientist.

The second comment deals with how one arrives at these thoughts; these books provided me with a review of the Coleridge method and the T. S. Eliot method. The Coleridge method is the seemingly desultory accumulation of a great many items of information. For example, Coleridge read about John Bartram's travels in the American South, accounts of travel in Georgia and elsewhere, and a multiplicity of others—bits and pieces

of information from hundreds of old books. I can't imagine that Coleridge had consciously in mind the creation of "Kubla Khan" or "The Rime of the Ancient Mariner." But it's easy to see, now that others have done the work on Coleridge's notebooks, that the two poems were really put together as it were, instantaneously, but not really instantaneously; rather by suddenly bringing together, in a harmonious way, multiple fragments from Coleridge's reading.

In the case of Eliot—I'm thinking only of "Four Quartets"—the feeling you get about how he put it together is that it was very methodical. He knew approximately what he wanted to do: to relieve religious anxiety and to meditate on time. But the lyrical apex of "Little Gidding," the fourth portion, "the dove descending breaks the air" segment, was in fact tinkered with in a very fussy and meticulous way, to produce what I consider, and I guess many people consider, to be a brilliant result. The point is that the ways in which one arrives at the poetic goal are very different. In that way, science and poetry are alike.

Sir Roger Bannister: I should like to follow what Gajdusek said and point out that Popper's ideas are not very helpful to someone who is a clinician and works in a very different way from the pure scientist. A clinician spends his life listening to patients and assembling data, rather as a botanist, trying to see what doesn't fit. When I was at Hammersmith Hospital many years ago, my chief had the facts in front of him of a new disease that was later recognized by Conn, whose name was then attached to the syndrome. The facts were there in front of my chief, but he couldn't fit them together.

The point I want to make about my own research is that for 20 years we have been assembling groups of patients who cannot regulate their blood pressure. The end defect

is the same—they don't release noradrenaline, but this is the end of a cascade of chemical reactions. Clearly, we were likely to find defects at different points in the cascade. But it took 20 years of assembling these patients, about 200 in the case of our group, and before finding a case that didn't fit also. There were reports of single similar cases in America and Holland, but it needed a technique for assay that was sufficiently sensitive to recognize that dopaminebetahydroxylase was

missing from this new group of patients.

I am suggesting that the discovery depended on a kind of serendipity, having a clinician carefully sifting a large mass of data, finding something that does not fit and then knowing what assay will prove it. This new disease has now been recognized, just in the last year, and the search is on for a gene now that can be identified to explain dopaminebetahydroxylase deficiency—the new disease.

that I am not the only example that illustrates the possibly beneficial affects of adversity.

Discussion

Howard Gardner: *I want to propose three interrelated concepts that can provide some labels for the things we are talking about. The first is that there are many ideas, and there are infinite amounts of data. How do we focus and pare down? And the concept here is "promisingness." What makes a set of possibilities seem promising? Mentors can be very important in this regard, especially for the student studying biology who may think of a thousand different possibilities, but who has no sense of which are promising.*

The second concept, which I think helps us think about promisingness, is what I call "fruitful asynchrony." This occurs when the new ideas do not quite fit into what you had before, but the distance and the tension seem to be fruitful. Sometimes the asynchrony is in the ideas themselves, sometimes in having switched from one area to another. Sir Andrew's switch gave him a fresh eye.

The third concept I take from Mihaly Csikszentmihalyi. His concept is called flow. And I think it's a better way of thinking about what was called "fun" this morning. Having studied many different populations, which range from artists to rock climbers to surgeons, but also including scientists, Csikszentmihalyi talks about the motivational benefit that occurs when the right mesh obtains between the challenges that confront you and the skills that you bring to bear. If the challenges are too great, you become anxious. If the challenges are too modest, you become bored.

Scientific work can be an upward cycle, with the challenges and the skills working in a nice mesh without your being overwound. This relates to Waelsch's presentation, be-

cause the adversity and the asynchrony that might have been enough to defeat many people may have come about because you found flow; a mesh that many other people might not have found. They might have been overwhelmed by the challenges, which, in your case, were not just intellectual, but also social and political.

Salome Waelsch: *I do not advocate adversity as the best means of promoting creativity, but I do feel that adversity can have—and I could mention many examples in support of this statement—positive effects, given certain prerequisites.*

Joshua Lederberg: *Of course, it's an uncontrolled experiment. And in your situation, there is no telling what further heights you might have achieved if you hadn't had to devote so much energy to overcoming adversity. One may never know that. I see so many different kinds of personality in science: There are some who throw themselves into their work out of some kind of neurotic drive, related to an optimum degree of stress. And others lead a nice quiet kind of existence. You would think that they don't really care that much about their research; but, by God, they just keep cranking it out, and do wonderful things one day after the next. Different people obviously require different kinds of stimuli in order to keep going. Adversity may have done wonders for you, and might have crushed others. When you say how adversity has been a help, can you spell that out a little bit more in your own history, or others that you are aware of? Just how did it interact with the way they went about their work?*

Waelsch: *How did it interact? Let me give you an example from my own experience, by mentioning Spemann, to whom I referred in my contribution here. He was perhaps one of the most outstanding experimental embryologists of this century, and received the*

Nobel Prize in medicine for his discovery of induction of the nervous system by the underlying mesoderm. When I went to him—because I had read about his work, and wanted to become his graduate student—he at first didn't want to accept me. Later he changed his mind, but assigned me the most boring doctoral research topic that he could think of. Even though I was aware of that, I also knew that I did want to study and find out more about experimental embryology in his laboratory, so I decided to remain there, in association with several post-docs and graduate students. Actually, I was able to learn there as much as anybody could, in spite of the fact that I never was very proud of the dissertation I produced.

Later, when I came to the United States, I did not succeed in getting a position, but I was permitted to work in a laboratory without a salary at Columbia University because my husband had a faculty position there. And so there also I had to find my own encouragement, and when I discovered a wonderful system for the problems that interested me; namely, the control and regulation of development, in the form of mutations that affected mouse development, I went ahead and overcame whatever obstacles were in my way.

I would just add that one of the reasons I was so excited about this system was the fact that Spemann, who was a vitalist, did not even consider the possibility that genes and their products were involved in the inductive phenomena and mechanisms that he had identified. He believed that a vitalistic force was responsible for the induction of the nervous system. So this inspired me to look for genetic effects, because I was convinced, even then, that genes were instrumental in controlling and regulating development. So I looked for a system where I could actually study this, and perhaps refute Spemann's vitalistic ideas by finding examples for the genetic control and regulation of development.

Frederic Holmes: *Besides the matter of adversity that you focused on, it seems to me that a stimulus to creativity, in your experience, is the effect of collaboration between people in different fields. You describe, but not in much detail, the importance of the early collaboration in the 1930s with L. C. Dunn. Could you say more about that; at what level you interacted? Did you just complement one another, or were there significant discussions between you?*

Waelsch: *That's a very interesting question, because Dunn was a really outstanding geneticist who, even though interested in development, knew very little about it. And I came with a very good training and education in developmental biology, but knew nothing about genetics, because of the prejudices in Spemann's Institute, where hardly a course in genetics was taught at a time when genetics was at least no longer in its infancy. And Dunn suggested that I would learn genetics in his lab, in return for making available my experience and knowledge in experimental embryology. And so this became (you are absolutely right and I'm glad you mentioned it) the most important stimulus for me to move into this particular area of research.*

Holmes: *And during those years, how much interaction was there between you? In other words, were there day-to-day discussions in which your two points of view resulted in ideas that neither of you might have had alone?*

Waelsch: *I would say in the beginning there were very frequent discussions. I remained in Dunn's lab for 17 years, and the discussions became less frequent and less important, and I would say that, after the first 5 to 7 years, I interacted with L. C. Dunn not much more than with other people in the department and the university. Of course, there I suffered from the fact that, whenever I asked why I could not be put even on the lowest rank of the faculty ladder, I was told*

(and at that time this could be said very openly) "You? A woman? Forget it." So this, of course, did not add to positive interactions.

Lederberg: *I would say in five years you've learned a little bit of genetics.*

Sir Roger Bannister: *I am a neurologist, not a psychiatrist, but this hint of unusual personalities being associated with creativity is very interesting. Much of the discussion this morning has been about high intellectual integrity, following logic, and being rational and unemotional. But of course, sometimes creativity is very far from this. It's worth considering to what extent the attachment to a particular hypothesis with a very personal enthusiasm gives a scientist the massive creativity to work day and night, and perhaps to defend this hypothesis because it becomes part of the scientist's self. This was hinted at by Sir Bernard Katz. There may suddenly become a point when the hypothesis is untenable, and then there is a great leap to a different hypothesis, which is then equally strongly defended. This is not a logical way of behaving.*

One of my teachers and predecessors, in Oxford, Sir George Pickering, wrote a book called Creative Malady, which was about another form of unusual behavior that can nevertheless be associated with extreme creativity. He wrote about Darwin, who seemed to be a hypochondriac. Were his headaches and lack of energy due to Chaga's disease? Or was this a neurotic way of keeping his life free for science? I would also like to raise the question of thought dissociation in great scientists. William Harvey spent half of his life prescribing herbal prescriptions that he probably knew had nothing to do with science, and yet that behavior seems to have been kept in a separate mental compartment, and he preserved his logic for his own experiments, which remained pure and had a total integrity.

Lederberg: *I've been having a very exciting reaction to, and same small discourse with, Nickles, about his paper on the romantic and enlightenment styles of scientific research. In partial answer to your psychiatric speculations, I'd suggest that to do creative work in science you have to be something of a schizophrenic. If you can't stand on the romantic foot part of the time, and on the critical, rational enlightenment side the rest of the time, you simply can't cope with all that's expected in scientific development. You have to build, create, fantasize, be the child, and play, in order to have the fresh and new and iconoclastic ideas. And then you have to work very hard to knock most of them down. Most of them are not tenable, and they require a detachment, a disattachment from one's own views, if you are to make that selection out of that universe of potentials that are then worthy of further work. The extent of that tension is not fully appreciated by many people outside of scientific effort, and the kind of cost that it can then involve. So I'm not sure that I would start with the malady as being the source of creativity. Nickles has wonderfully summarized those strands when he talks about the romantic and the enlightenment effort; I'd just like to add they have to be internalized in the one individual. Is that others' experience?*

Baruch Blumberg: *The question of the rational and the intuitive often comes up in the work of the scientist who is also a clinician. Scientist-clinicians may be faced with a dilemma when engaged in a therapeutic trial. When administering a treatment to a patient, the physician is anxious to ensure the best possible outcome. Hence, he or she will be enthusiastic when administering a drug. On the other hand, if the effectiveness of the treatment is being evaluated, objectivity is required.*

I had a personal experience with this many

years ago when we were evaluating the efficacy of gold treatment for rheumatoid arthritis. I knew from my prior experience with rheumatoid patients that enthusiasm and positive suggestion by the physician can create a wholesome therapeutic climate. But, I also had to maintain objectivity in analyzing both the laboratory results and the subjective testimony of the patients.

Jochen Schaefer: I would like to come back to Waelsch's presentation. As a physician, I wondered if you would think that there are "graded adversities?" Could you say that it has to be a "vital adversity," or could it be less than that, say resistance, or merely something like lack of encouragement?

Waelsch: Do you mean to say how strong does adversity have to be?

Schaefer: Yes.

Gardner: How sweet even.

Waelsch: How sweet? Well, I think the obstacles have to be pretty strong, and I don't talk about little things, for example, disagreements, or something like that. It doesn't really have to go as far as affecting your survival, but I mean something close to it. I am talking about strong adversity, yes.

Howard Gruber: I want to make a comment and ask a question. It seems to me that Waelsch's story illustrates how the creative process is to some extent buffered and protected from society. Obviously, if we look at the statistics, German science suffered from the adversity we are talking about. The events in Germany were not good for science or for German science. At the same time, you as an individual managed to find some coping mechanism, some way of building a world in which you could do what you wanted to do. How did you do that?

Waelsch: Do you mean how did I manage to continue in science in the United States?

Gruber: No, I really meant while you were still in Germany. While you were developing, and you encountered the two forms of adversity you mention.

Waelsch: I'm not sure I understand the concrete nature of your question.

Gruber: As somebody interested in the individual case, I would like to know how you developed the techniques, the miniworld that everyone actually functions in, so that you were protected from these adverse social affects.

Waelsch: Well, that was perhaps due to the fact that I was sufficiently motivated (or crazy as Josh calls it) to want to go on into science in spite of the difficulties. This made me find opportunities to do that. To go to university was easy in Germany, because university was free and anybody could go. To be accepted in a laboratory of your choice was more difficult, and I just decided to compromise and accept a thesis problem that did not excite me, for the sake of being in an environment that excited me very much. And then I left Germany after I got my degree.

Lederberg: How did you leave?

Waelsch: How did I leave? Well I left because I was very fortunate to be married to Rudolph Schoenheimer, who was a young assistant professor at the University of Freiburg. Fortunately, Hitler's first anti-Semitic decrees were directed against university professors—Jewish university professors—who were dismissed on April 1st, 1933. I call this "fortunate," because it saved our lives; later you couldn't leave as easily as we could. This was just good luck.

Lederberg: I think that was the kind of tale you were looking for wasn't it, Howard (Gruber)?

Gruber: Well, I'd like to know more about it, but I don't know how to ask the right question.

Waelsch: *Well, it was similar to what Sir Bernard talked about; one had to leave in 1933 if one was at the university.*

Gruber: *I wasn't really asking about how you migrated, I was asking about how you build an inner world in which you can work when you are under attack.*

Gardner: *I think that part of what she is saying is she was able to tell herself a story, to generate a narrative and a meaningful context whereby the oppressive forces were diminished in importance and the opportunities were great. I think what's surprising to us is that somebody under those circumstances can tell themselves that kind of. . . .*

Waelsch: *Yes, but I'm not the only one. I mean, you see some such people here, but there are many others.*

Lederberg: *You're raising, and others have raised, a very interesting question. The remark was that science in Germany was destroyed, and certainly it was; but the science of the emigres from Hitler's Germany had such an extraordinary flowering elsewhere in Europe and in America. One almost wonders if they didn't superexcel, having emigrated, compared to what they might have done if they had been in the. . . .*

Waelsch: *This is a very valid point, and of course, in Europe, you might think of Hans Adolf Krebs—Professor Holmes, I am sure, has also thought about him—and the same applies to Schoenheimer. The question is to what extent was the fact of having to emigrate and then being exposed to particularly stimulating environments responsible for their success? I don't know who would have succeeded equally well, had they not emigrated; as you said, there is no control experiment. I had actually experienced a very stimulating—intellectually stimulating—atmosphere. But for the first time I found myself in a freer society.*

Lederberg: *If we still cautiously remember that it is an uncontrolled experiment, I am going to bring up a Toynbeen proposition, and that is, it's not just adversity you are speaking of, it is almost contaminated by another phenomenon, and that's a kind of cultural hybridization. And I just wonder if the mix of the German academic culture, and then the transplant into a different environment, may have added important ingredients quite apart from the adversity questions. I am just putting this up as a speculation, but the flowering of German emigree science is certainly a stunning phenomenon in the history of 20th-century science. Whether it could have happened anyhow in situ, we just don't know, but it raises an interesting question.*

Gruber: *In psychology, the opposite phenomenon happened. At the New School for Social Research, in New York City, which was known as the "Weimar Republic," there was a concentration of gestalt psychologists who flowered by virtue of not interacting very much with American behaviorism and other trendy tendencies.*

Blumberg: *I had occasion several years ago to work with the United Nations High Commission for Refugees. This remarkable organization was created after World War II to assist with the enormous problem of displaced persons and refugees that resulted from that war. They have continued to deal with the other refugee problems that have arisen since then. Workers in this field have said that refugees, on average, tend to be more successful after resettlement, than the populations from which they emerged and often, after a period in their new homeland, better than the populations among whom they are resettled.*

Lederberg: *Is it selection or induction? These are voluntary emigrations; studies have indicated that emigres tend to be a select group. I don't know how you can do a better con-*

trolled experiment. When an entire population has been evicted, then there is no selection.

Carleton Gajdusek: *Waelsch discussed an interaction which occurred at a critical point with two people—Oscar Vogt and then Curt Stern. Would you add anything further about this?*

Waelsch: *Well that is very simple really. If you are trying to ask me whether Oscar Vogt's attempt to encourage me had a negative effect on me, the answer is "No." Even though I am stressing paradoxes, I do not want to go to that extent. Curt Stern, on the other hand, had a very stimulating effect on me with his negative comment, which I think I mentioned . . .*

Gajdusek: *That's why I ask, because it was ambiguous as you put it in your presentation.*

Waelsch: *Unfortunately, Curt Stern also had to leave Germany later, but I still remember his comment to me to this day. When I walked out of that building, I was determined not to give in and, at somebody's suggestion, I went to see Oscar Vogt, who encouraged me and urged me not to give up. He introduced me—I don't know whether I mentioned that—to Delbrück, and here was the possibility of working with Delbrück, who had just joined him as a young post-doc. Timofeef-Ressovsky was there also, and Delbrück learned his first genetics from Timofeef-Ressovsky. Well, Vogt had a most encouraging effect on me.*

Gajdusek: *Well, I brought that up because of a comment I wanted to make; I am a decade and a half behind you, non-Jewish, and I come from an American Slovak-Hungarian family. Before I was 20, I worked with Rudolph Schoenheimer, your husband, and Viktor Hamburger. Max Delbrück was a close friend and an inspirer, and Oscar and Cecilia Vogt were well-known to me, and in my late 20s, I collaborated with them. I could go on*

to add Bela Schick, Michael Heidelberger, Viktor Weisskopf, Werner and Gertrude Henly, etc., etc. So you mentioned about eight people, or ten, in your essay. And, 15 years behind you, and in a different setting, more than half of them were heavily involved with my life before I was 25. Curt Stern I worked with as a teenager; I worked with Viktor Hamburger as a teenager; I worked in my early 20s with Max when he first came over; and with Oscar and Cecilia Vogt and Carl Cori a bit later. So I think the point I am trying to make is, that it's not me personally. A point that we are glossing over is this matter of what effect personalities have on schools of science. I have never met Salome Waelsch directly until today, and yet we are very close to about 10 of the same people.

Waelsch: *But I have admired you for a long time.*

Gajdusek: *But that's no accident; I think that's one of the things you have to think about. This is no accident. I didn't grow up in Germany and that part of the world, but these many she has mentioned were part of my life before I was 25—directly as friends, on a first name basis.*

Lederberg: *Was Stern as hostile as you are making him out to be, or was he in some odd way trying to be friendly to you?*

Waelsch: *No, no, he wasn't hostile. No. He was, well, this brings up a problem that doesn't belong here, that doesn't need to be discussed here. It was the problem of the type of German Jew who felt that he was not a Jew, but a German, so he felt what would hurt me, would not hurt him. He was totally unwilling to identify with other Jews who, like myself, had parents who had come from the Ukraine, whereas all his ancestors had lived in Germany for generations. So he considered me undesirable at that time—that was before 1933—as undesirable as the Nazis considered him and me later.*

Lederberg: *That's a very poignant reflection.*

Lord Butterfield: *I am not sure whether this is the right moment to raise the questions I have in mind. Like others, I am anxious to discover what lessons we can learn here today for those research managers with responsibility for encouraging creative people. Katz mentioned that being a refugee and therefore being left undisturbed, allowed him to pursue his work on his "little action potentials." Research managers need to be aware of something that is frequently of great importance to creative people, namely—peace. This is part of the ambience that people with managerial responsibilities should strive to create in laboratories today. I wonder where there is enough peace today to allow Huxley to go to The British Museum to look up his grandfather's papers? Wouldn't he be pressured nowadays to publish that paper that, in earlier days, he could hold back for another year?*

And exactly how important is the reverse atmosphere, not peace, but competition? Jim Watson's story of the double helix implies he was in a race against Linus Pauling. I'd like to know how real that was, and whether it was an important stimulus to him and the others. I recognize in the first case—creating the peaceful scene—one is allowing scientists to accumulate data and follow up the leads, whereas in the other, the atmosphere of a race may stimulate thinking and analysis. When we are involved in appointing managers of research in universities, usually as heads of departments, or even the secretaries of research councils in government, do we take into proper account their track record in these ambience matters?

Conversely, do the creators here today have any strong views about the importance of the ambience in their departments or laboratories? They may be so busy that they are not conscious of it. I would be interested to

know whether anyone else thinks this is an important factor in creation. Richard Adrian has touched on the fact that we seem to be in a rather puritanical situation, namely that we shouldn't enjoy our work, particularly if we're faced with competition. . . .

Lord Adrian: *I only said that our paymasters think that.*

Lord Butterfield: *Yes, apologies, our paymasters, and in the present circumstances, I believe we are going to need managers and leaders who can hold an umbrella over the folk who are doing the creative work and enjoying it, making quite clear that this is the first phase in a process that will, presently flower, perhaps only once in 10 times, into a special new product. I don't know whether Jim Black is going to talk about that any time.*

I hope that somewhere along the line today there might be some discussion about what qualities we might be looking for. When I said goodbye to my undergraduates, I used to say: "Well now, you are going out into the world; don't forget Pythagoras's four great Greek virtues to be sought in leaders—knowledge, courage, judgement and self-control." Should we be urging vice-chancellors to be looking specifically for the same qualities when they are appointing heads of departments? I'm sure, in research, the leader will need to have an additional quality, namely, the ability to create the right atmosphere.

When I was starting off at Guy's in the late 1950s, somebody thrust into my hand a remarkable little book by a Dutchman who was experienced in management and organization of research. I read it and reread it, and I was delighted because, at the end, the last few paragraphs said, "it all comes down to whether or not you can create an atmosphere of love in your research department." Well, that's a thought, isn't it?

Sir Christopher Booth: *Can I just try and*

answer that, then make a point? My own view of any research director is that his fundamental job is to create an environment where genius can thrive. There are also certain laws he has got to follow. The first is that the half-life of knowledge is very short nowadays, so you've got to turn things over very quickly, and universities are very bad at doing that, and research institutes are better. And the final thing is to remember that Gresham's law of money applies as much to research institutions as it applies to finance. Bad drives out good, and you must therefore weed out the bad as quickly as you can.

But I think you've raised another issue, and that is Germany. The thing that always intrigued me about the German story has been the original foundation of the University of Berlin by Wilhelm von Humbolt in 1810. He had this concept of "einzamkeit und freiheit," an idea that spread through Germany in that romantic period. von Humbolt was a friend of Beethoven, of Schiller, and of Goethe. His concepts developed into a university sector that has spread through the world, particularly in the United States. And then it just got destroyed by anti-Semitism.

I'm still interested in the relationship between individuals working in this sort of environment and the influence of institutions. I'd like to ask our Chairman this morning, particularly, how genius so flowered at the Rockefeller Institute in New York? I don't know how many Nobel Prizes have been won there now; the last time I counted it was 16. Is that correct, or have you got more? 19. Can you answer that? What is the impact of institutions? Or is it because the Rockefeller Institute did specifically recruit individuals who were great?

Lederberg: *I don't think there is a simple answer to these questions. You have to look at individual personalities to have a sense of what it is that will drive them. I think our*

full professors and heads of laboratories, for the most part, have been through the fire; freedom and an infinite time to develop their ideas is probably the best strategy. Now that science has become so popular, especially in the United States, and we are recruiting really rather a large number of people going into it, I'm not sure that I would apply the same criteria to everyone. There are some, certainly among the younger group, who would need some constant oversight, and have to come back to take their exams from time to time, or they will feel very comfortable about doing very routine things, and possibly not even working very hard, and so on. You really have to look at the motivations of the individual. Those who are really self-motivated—and you can only learn that after a while by some experience—you had better leave them alone for as long as is necessary. But others are going to need a prod from time to time, so we have in effect a mixed system. I really think that is what works out the best.

Blumberg: *I would like to comment on the issue of a creative environment. A large part of my research has been done in the field, and that of course, is also true of my friend and colleague, Carleton Gajdusek. Field work is very demanding; there is a great deal to do, often under trying circumstances, and with insufficient time to do it. Much of the time I was alone. This gave me ample time to think and to reflect. One was also very close to the data, in the sense of actually being with the people under study, and experiencing the same environment that increased their disease risks. The impressions were vivid, even dramatic, and the scene foreign and exotic.*

Scientists have written of this atmosphere. Wallace was said to have formulated the notion of natural selection when he was suffering from a bout of malarial fever when he

was in the East Indies. Humboldt's writing have many references to the scientific stimulation he gained when on his amazing travels in the equatorial parts of South America.

Another quite different atmosphere prevailed when, in 1964, I first came to the Institute for Cancer Research (now the Fox Chase Cancer Center) in Philadelphia. I was the recipient of a seven-year research grant for which, amazingly, there was no research goal stipulated. It provided us with a great sense of freedom to pursue any line of work we thought useful, with ample opportunity to find our way unhampered by restrictions and caveats. Further, the then Director, the late Timothy Talbot, was a remarkable leader of a research organization. He placed the highest premium on independence and freedom of action.

Sir Raymond Hoffenberg: *I was just interested in the discussion we have been having about the interaction between influential scientists and their young proteges. And the tribute is always paid to the influences, and I was just wondering to what extent people select out the people, the senior individuals who are going to have such a big influence on them. I'm particularly thinking of that with Carleton Gajdusek, because here you were as a very young man, in your teens and early twenties, seeking out people, presumably before they sought you out, so you must have had a certain innate ability and a very clear idea of who you wanted to attach yourself to.*

Gajdusek: *I run a big laboratory with 20-odd post-docs, and they come and seek us out, many of them. I often deflate them in an unkind way by throwing names out. Quite frankly, I list more than 20 Nobel laureates whom I knew well personally and in many of whose labs I worked, more than a decade before they ever got the Nobel Prize. I never entered their lab after their prize.*

In other words, I never worked with John Enders after his Nobel Prize, nor with Linus Pauling after, nor with Max Delbrück. When I knew Josh, he was following me to Mac Burnet, and it was before his Prize, right? And so essentially there is a true matter of selecting out, but there is a shrewdness about it. I don't think you go running to the fort of older people in the field. Let me tell an anecdote.

Benoit Mandelbrot was the junior of the post-docs in the Golden Age of Cal Tech. He spent more time with my mother than I did after my teens. At that time he was working with Albert Einstein, as still a pre-doc, at the Princeton Institute. And to my mother's chagrin, he came in one day saying he was giving up hanging around with these non-creative old boys who were just reminiscing about the past and had no new ideas. He was talking about Albert Einstein, with whom he was working! He went off as a pre-doc to Paris and subsequently, of course, to MIT, Harvard, Yale, and he became a principal IBM researcher. He is the fractal man, who produced the whole concept of fractals. So there is another example—a pre-doc in his 20s, and the Princeton Institute of Advanced Study with older famous burned-out people was not the place for him to be.

I wanted to make one other comment to Lord Butterfield. I think it's way off what I thought we would be discussing, but just as I left NIH there was a near disaster. Of all the people in the United States who have been granted an enviable privilege, it is those Hughes Fellows—all Ph.D. and M.D. candidates, many near to getting both. And then they have been plucked out to do anything they damn please—roam around, Chinese cooking, think, work with anybody at NIH, a huge travel budget to carry their work to anywhere in the world. In my day, there was no such luxury available in the United States—probably in the world.

And to my amazement, after watching how productive these fellowships seem to be, I learned about a month ago, from a group of these post-docs, that some administrator in Purcell's machinery of the Hughes Institute has been trying to discipline this gang of Hughes Fellows. To get them back into engineering, into molecular labs where they will keep their noses clean, and learn all the techniques for one solid year of doing humdrum machine work. I couldn't believe it, but in fact, that was happening, so your question is quite valid.

Here we have probably the most luxurious pre-doc fellowship in the world, and already some administrator has got his fingers on it, and thinks that these people, who are bright enough already, can improve by working only in my molecular lab, where they haven't got anything more to do than a machine could do. And somebody wants them to putter around in there for a full year and not use their minds for more widely intellectual exploration. He objected to their doing field work, epidemiology, clinical-laboratory correlations rather than pure molecular engineering. I was indignant about it, but I think it shows how confused people can get in the administration of creative science.

Lederberg: *Carl, I think you must have quite unique experience. I can't imagine any other teenager even knowing whom to approach, much less succeed, in doing so in the way you did in such an expansive fashion. I really marvel.*

Gajdusek: *Now let me not brag, but the fact is, Linus first came to me. I was working with Michael Heidelberger as a young pediatric intern at Columbia Presbyterian Medical Center, Babies Hospital. I was 22. And I had two "lackeys" in the lab, who were the two immunologists, young Myer and Kabbut, since I was the bright white-coated pediatric intern with clinical ward duties and I worked at night, leaving the laboratory for Heidelberger's post-*

docs to clean up. Linus Pauling visited because of his interest in acute glomerulonephritis from which he had suffered. He had heard that I had a new research approach to glomerulonephritis going. He heard that I had planned to be at Caltech already as a teenager, with Robert Milliken, who was showing his age. He asked me to come to his lab. Now, I wasn't that bright, I had done nothing yet, I had not published a paper, but Linus Pauling was in the intern and residents' quarters, asking me to join him at Caltech because I had a bright idea for new studies on auto and chronic glomerulonephritis. How does that happen with people? I am seeking out promising young American investigators, even undergraduate students, the same way. Recruiting them on my own initiative.

Lederberg: *Well, I think that may be a greater mark of Linus's genius than anything else he ever did.*

Waelsch: *Were you not a teenager when you came to Columbia?*

Lederberg: *Yes, but I didn't know whom to go to. It was just pure luck that I fell into that. I'd never heard of Francis Ryan before I came to Columbia, but had all the wonderful benefit of that experience—pure serendipity.*

Waelsch: *I would like to add something to this paradoxical proposal that I made here, namely that I feel—and listening to Carleton Gajdusek made me think of that—I feel that a creative scientist should also be a rebel, or at least something of a rebel.*

Booth: *That point was made by Peter Medawar in one of his books, in which he said the research laboratory must have a Maoist element, a sort of Maoist microcosm and not be an entirely ordered scene.*

Kenneth Schaffner: *There has been a wide variety of themes we are covering in this discussion, and there was one that I want to go back to very briefly. And it came up, I think, in*

connection with Howard Gardner's comments about some of the different approaches that one might take to identify important factors. It seems to me that, though we've touched on such general issues as the environment, the problem comes with individual differences with respect to their motivating influences. That one possibly understudied feature is the heuristics, to use Nickles' term, that the mentor contributes to the student. That's something that philosophers of science have not spent much time looking at. When they have, it has been people like Michael Polanyi who have suggested there is a kind of tacit and passive dimension to such learning experiences where you can't really get a hold of it. It suggests, on the basis of the comments that a number of people have made here, that learning those heuristics is a rather crucial learning experience. It may be difficult to make them articulate and explicit. Nonetheless it seems to be an area that it probably understudied.

Lederberg: Well, to put a footnote on passivity, I think a good laboratory director and mentor learns as much from his students as vice versa.

Sir Andrew Huxley: I have never felt that I have had a long-term plan that I have followed. I suppose at any time one has ambitions: When I went into muscle, it was the contractile process I was primarily interested in. But as I said, I did not have any foresight about sliding filaments. I did say that it came to me fairly suddenly when we saw a dense line where the thin filaments were overlapping in the middle of the A-band. The other things came gradually. In a general way, as I said, there was an analogy with Bernard Katz's quantal transmission: (a) whether it is quantal, and (b) what the nature of the quantum is. Well, there was (a) the sliding filament idea itself and (b) the question "What makes them slide?"

And I think almost everybody now believes, as I did from our first publication, that

within the overlap zone, there are little force generators acting more or less independently. That was immediately suggested as soon as we got on to sliding filaments, because I was familiar with the work of Ramsey and Street, which showed that the force declined with the initial length of the fibre, more or less in proportion to the amount of overlap, and that seemed obvious as soon as we got onto the idea of sliding filaments. But it was necessary to have been aware of the work of Ramsey and Street in advance, and having a full background in physiology was important for that. I've no idea whether Hugh Huxley was aware of that paper, because he came into biology from physics, and I suspect it is very important to have a broad background in biology so as to make it possible to put things together in that way. A thing may seem obvious when you've got the background, but if you haven't got the background, it's not obvious at all.

But as regards details, as Katz said, yes, we felt in control. We planned limited steps forward, but no great strategy. We have had a strategy over working out the transient responses of stimulated muscle, but that was a late stage. I suppose the same was true when Hodgkin and I were working out the consequences of our electrical measurements on the nerve membrane. It was a long interval from making the measurements to final publication. And at that stage I think there was a strategy, but that was after the general framework had become clear. And it was the same with this work on the transients in muscle.

Waelsh: It seems to me that any strategy or control is determined by the nature of the experimental material in which you are interested. For instance, in my case, the experimental material is provided, to a large extent, by nature, because I am interested in the study of spontaneous developmental mutations, which cause abnormalities of development on any level—molecular, biochemical or morpholog-

ical. I am therefore limited to controlling the strategy of experiments that address questions of the mechanisms by which these abnormalities arise as a result of mutated genes. But I do not control the results of the mutant effects themselves. Many of these are in a way experiments of nature, which I am trying to analyze and interpret. I am trying to say that in everyone's case the answer to your question would be different.

Lederberg: I think you were expecting that answer.

Holmes: I would like to return to an earlier point. I would like to know more about the stimulating factor of adversity. You are now looking back on those effects of adversity in the light of having been eminently successful in the long run, and you create the appearance of having in the past been indomitable in the face of the adversities you encountered. They never seemed to threaten to overwhelm you. Were there in fact no times when you were ready to give up or think that there was no chance for you to succeed as a scientist?

Waelsch: I do not recall that I ever was ready to give up. I also consider myself a rebel, and therefore, when obstacles were put in my way, I tried to overcome them, possibly with unconventional means. I know I was never discouraged. I'm not discouraged now. Now my age is a great factor of adversity, but I'm not discouraged.

Holmes: Well, part of the answer is that it has a lot to do with the nature of the individual that the adversity affects. Whatever degree of creativity you have, this sense of being very resistant to discouragement is an important part of your success.

Waelsch: Right; that's one of the primordial

requirements really. I would like to quote Bette Davis, who had a big obituary in the New York Times last week. She called herself indestructible, and that's the word often used to describe me. I suppose it means that one just overcomes everything, but without things to overcome, you don't become much of a person, do you?

Sir Roger Bannister: We have talked about some negative qualities that are compatible with success and creativity. I should like to ask our creative experts whether they think there is a noble quality of openness, a willingness to share ideas even with rivals, that is important in the modern scientific world? This contrasts totally with a competitive, secretive atmosphere, which sometimes exists at a lesser level. Would they like to comment on this noble quality associated with greatness in creativity?

Lederberg: There again, we have one of those ambivalences in sources of tension. The only way that a scientist can reap any fruit from his effort is eventually sharing it, so it's just a matter of time whether it's this month, or next month, or day to day. The system does impose the necessity to publish, and I think that's really the more important answer. Some are going to be more cooperative in the short run, some take longer, but it doesn't really matter that much.

Huxley: I was brought up in a tradition of openness under Alan Hodgkin, and I regarded it as the normal way to proceed. The one thing I really dislike about *The Double Helix* is that it gives the public the impression that this sort of secrecy and competition is the way that most scientists work. I have come across very little of it in my own career, and I think it's terribly important to be open. I also think it's important to be open when you are acting as a referee of somebody else's papers and, whenever possible, I disclose my identity.

Blumberg: *Certain areas of the medical community were very interested in using hepatitis B vaccine, for example, renal dialysis units.*

Butterfield: *I hope Howard Gardner feels this is indirectly relevant to his presentation.*

Gardner: *I've been taking notes.*

Lederberg: *Something close to what Gardner was saying. He made a very important distinction between field and domain. What you are bringing up is choice among various research strategies. Some people will make very successful careers by specializing in one area: make it their own, in the sense of knowing much more about it than anyone else; develop all the techniques that are most appropriate to it; and do splendid work. Charlie Yanofsky has built a magnificent career studying one enzyme for the last 35 years, the tryptophane synthetase. Others feel, "Well, I get bored after three or four years on a particular problem, and I like to skip around."*

I'm not suggesting that one paradigm is better than others; there are matters of temperament. Personally, I most enjoy trying to find ways in which domains ought to intersect with one another. And the feeling of paradox is very similar when you have the sense that there does not yet exist a genetic embryology, or in my own experience, a microbial genetics. It's very much akin to the paradox of finding that there are some facts missing, or they are discrepant with one another. I have a great urge to clean it up. You may have a certain efficiency, because those interdomain areas are likely to be less thoroughly investigated; because not so many people have that temperament, or for other reasons may lack the ability to get in there.

My own observation is that interfield studies have had the most creative impact per unit of effort expended, but it may just be because they are relatively less crowded. But it is very much a temperamental matter from one kind of research style to another. One

can be accused of being a dilettante, skipping around that way, and maybe that's right. It may also be successful.

Gardner: *Your comment, particularly coupled with Holmes' much earlier question, leads to this notion: If we can speak roughly about some individuals as having a more synthesizing inclination, and others as having more of an analytic inclination, then the model I presented today was really much more helpful for people who are of the analytic than it is for those of the synthetic frame of mind. And I have to confess, this was in part a direct result of my preparing for today. I wrote this paper with my colleague six months ago. But when I began to read the papers here, I said to myself "Goodness, these authors are all about something very, very different from what I'm talking about." Namely, I think Freud and Cantor were much more of the synthesizing kind, trying to bring lots of things together. So I said, "Gee, this isn't relevant for the prototypical paper here, I need to focus much more on analysis and discrepancy," and I may have gone too far. You almost need to have a somewhat different story to tell about people who really see connections among domains as you've mentioned, and as Dr. Waelsch has done. I don't think it's fair to take the notion of the discrepant element and distend it to such an extent so that it covers all analytic and synthetic activity.*

Lederberg: *If I could have a quick word on a research method to study takeover by a field. Let's look at neologisms, and contrast the meaning promulgated or intended by their original author when first introduced, and what has then happened in subsequent years. I can give you a whole shelf full of examples of how even the definitions of terms get to be taken over by a field, so they are almost unrecognizable.*

Nicholas Russell: *Dr. Gardner, you have given*

us a model that you say we might find helpful, but you have already suggested that, having read the contributions here, you are thinking it might be necessary to change the model. Can anybody devise a sufficiently general model? And if you did have such a general model, how could you test and apply it? I have tremendous difficulty relating very specific statements made by scientists about how they do things, and psychological and sociological theories about how scientists should or do operate. I can't connect the two things. They seem to be almost on different planes. Obviously for one to converse with the other, there has to be some kind of interface. Can you help me with my problem?

Gardner: I think that for social science to aspire to the kind of model we take from the natural sciences—in physics and increasingly from the biological sciences—is a mistake. And I think it has done us a lot more harm than good. I talked about the Eysenck intelligence test but that's merely one of a hundred examples one could use. My aspirations for the social sciences are much more modest. I think that it is reasonable to expect at this point certain useful terminologies, whether or not they are neologistic—and certain frameworks that might help you to bring more sense into something that has been an even greater mess before you had a framework available. I don't believe that you could have a single gritty theory of creativity. I do believe

you could have an ensemble of local models that would explain enough, or at least describe enough, to make it worth having that ensemble.

In the beginning of my paper I talk a bit about how we built from case studies. But unlike the humanistic scholar, who is interested purely in the individual case study for its own sake, be he or she a literary critic or historian, somebody with the social scientific cast of mind wants to take a look at a number of individuals working within a domain to see what kinds of interesting parallels might exist, and then begin to span domains to see whether any interesting generalizations obtain across them. I get some "flow" from doing that, but I would be the last person to say that, if you don't get flow from it, you should have much interest in it.

My own experience with scientists is rather like my own experience with artists. Some artists find it absolutely compelling to try to understand their own creative processes so to speak, and others find it a waste of time. And others find it injurious to their mental health. I think we are dealing here with a personality variable, and I would say the same thing about attempts to explain creativity. That's why my first remark this morning was that it was interesting to see how few of the attendees who had read Popper actually said they had gone to him because they wanted to understand their own creative processes. I don't think it is a high motive on the part of most scientists.